MINOR CONTRIBUTIONS.

Studies from the Laboratory of Experimental Psychology of the University of Wisconsin.

By Joseph Jastrow, Ph. D.

By way of introduction to the first appearance in print of this Laboratory a few words may not be out of place. The Laboratory was founded in connection with the chair of Experimental and Comparative Psychology, established in the fall of 1888, the duties of which, I at that time assumed. The object of the Laboratory is primarily to give opportunity of demonstrating the chief points in a course in psychology, and of allowing students to test for themselves the simpler results of the methods of observation and experiment, and secondly to provide facilities for advanced and original work. In the programme of an American college the former end must stand out more prominently than it would, for example, in a German university. The original work in turn must be more directly under the guidance and control of the director of the Laboratory and the themes suited to the capacities and available time of the student. These, as well as the necessarily slow growth of a somewhat novel department, form the chief,—but I hope and believe constantly decreasing difficulties in the way of giving the actual a reasonable approximation to the ideal. My policy, however, is not to bring the researches conducted in the Laboratory under any one general scheme, but allow them to be suggested by the interest of the student, by the facilities of the Laboratory, or by the fluctuations of interest in the psychological world.

Regarding the present contributions, I have only to say that they will give evidence of some of the limitations under which they were worked out, but that I thought it wise not to delay their publication in anticipation of future results, but to send them forth as they are, to excite whatever interest and encourage whatever research they may. They are directed mostly to points connected with the problems of the psychophysic law, and may, perhaps, contribute a little toward bringing a much desired unity of conception into that vexed field.

Under the appropriate heading are mentioned the names of

the students who obtained the experimental results either directly with me or upon one another under my guidance. I trust to be able to continue these contributions at about yearly intervals.

On the Psychophysic Series.

The conformity of the magnitudes of the stars to the series demonstrated by the psychophysic law still remains one of the most striking applications of this law as well as an important piece of evidence in its favor. The stars were arranged in magnitudes on the basis of their naked-eye appearances, and at a time when any objective determination of their brightness was impossible. It is natural to suppose that the astronomers had in mind a sort of series in which the average stars of each magnitude should be separated by equal differences of brightness; i. e., by equal differences of sensation. When, now, we come to compare this psychic series with the physical series formed by the photometric determinations of the average stars of the several magnitudes, we find that this latter is approximately a geometric series with an average ratio of 2.5, for the first five or six magnitudes. To the arithmetical series of sensations separated by equal sensory differences there corresponds a geometric series of stimuli separated by a constant ratio; and this is the relation most closely answering to Fechner's formulation of his law. It is the most direct method of testing whether the sensations increase in arithmetic ratio as the stimuli increase in geometric ratio; i. e., whether the sensation increases with the logarithm of the stimulus. In this Journal, Vol. I, pp. 112–127, I have traced in detail the agreement of the estimations of star magnitudes with the phychophysic law, and in the present study my aim is to test whether this method can be applied to other fields of sensation, (for this, to my knowledge, has not yet been done), and with what results.

A.—Visual Extension.

My first attempt was with spacial relations of vision. A very large number of thin sticks varying arbitrarily in length from a few millimetres up to about 300 millimetres were mixed together in a random order; and the problem of the subject was to arrange these sticks according to length in a given number of classes or, to keep the comparison, of magnitudes. For this purpose I had made a frame with nine square openings, each one foot square, and with a bag hung within each compartment.[1] The whole was conveniently supported so that a person could sit with the sticks next to him

[1] The apparatus was constructed from a grant made me by the Elizabeth Thompson Science Fund, which I again gratefully acknowledge.

and sort them out according to a general impression of size. But one stick at a time was seen, and as soon as it was thrown into the bag it was lost from the subject's view. At first one's idea of the average length of each magnitude is vague, being founded only on the lengths of the extreme sticks that had been shown at the beginning of the experiment; but as one goes on his idea soon becomes clear, though puzzling cases of sticks just on the boundary between two magnitudes will always occur. When several hundred sticks had been thus assorted they were taken out and measured and the average length of the sticks in each bag computed. If the psychophysic law holds true of sensations of visual extension when thus tested, then these averages should form a geometric series with a constant ratio, just as do the photometric determinations of the average star-magnitudes. My results include the records of five persons sorting the sticks into six divisions, and of five sorting them into nine. Regarding the former, one observer declared himself dissatisfied with the result owing to the changing of the standards during the operation so that too many sticks had been thrown into the "longest" compartment. On examination this was found true and I have therefore omitted his result; the omission, however, does not effect the average result. The other four results are:

I.	Number of sticks,	79	133	89	70	56	88
	Average length,	18.5	55.6	97.7	146.8	194.7	251.1
II.	Number of sticks,	122	137	113	57	61	25
	Average length,	25.1	61.8	124.6	195.6	239.0	278.9
III.	Number of sticks,	236	65	59	59	60	37
	Average length,	45.9	106.1	147.5	184.2	231.3	273.8
IV.	Number of sticks,	200	79	103	52	51	50
	Average length,	36.8	90.1	142.3	200.7	239.9	275.2
Ar.	Number of sticks,	159	104	91	60	57	50
	Average length,	31.6	78.4	128.0	181.8	226.2	269.8

The last lines of figures represent the averages of I, II, III and IV. The following is a similar result for the sorting into nine magnitudes by five other observers, and their average:

I.	Number,	116	87	70	43	51	42	29	40	27
	Av. length,	62.8	84.5	111.7	139.5	164.5	187.8	215.5	233.5	252.2
II.	Number,	57	63	35	47	53	57	81	51	63
	Av. length,	44.5	66.6	73.5	92.0	107.4	136.2	164.9	212.8	231.8
III.	Number,	36	85	83	86	71	44	37	41	21
	Av. length,	40.3	65.4	86.6	121.6	156.6	190.3	218.2	235.0	254.8
IV.	Number,	15	27	39	67	86	116	78	69	11
	Av. length,	33.6	41.9	57.6	71.4	95.1	139.0	186.3	235.4	256.3
V.	Number,	56	80	82	63	66	58	56	27	18
	Av. length,	43.2	67.7	91.5	119.1	151.0	177.3	221.0	240.4	256.3
Ar.	Number,	56	68	62	61	65	63	56	46	28
	Av. length,	44.9	65.2	84.2	108.7	134.9	166.1	201.2	231.4	250.3

We need only compare the successive differences of the several magnitudes with their successive ratios to obtain an answer to our problem. Doing this for the average result we have:

Average difference,	46.8	49.6	53.8	44.4	43.6
Average ratio,	2.48	1.63	1.42	1.24	1.19

Average difference,	20.3	19.0	23.5	26.2	31.2	35.1	29.2	18.9
Average ratio,	1.45	1.29	1.27	1.24	1.23	1.21	1.15	1.08

In the division into six magnitudes it is quite clear that we have to deal with an *arithmetical* and not a geometrical series, or that the result is quite different from the result with star-magnitudes. In the division into nine magnitudes the difference between the two series is naturally considerably less, and so a decisive result the more improbable. The averages are considerably more irregular,[1] and the process is in every way more difficult. But if we regard the individual records as well as their average, we find that the balance of evidence tends towards making this also a coarsely approximate arithmetical series. If the series tends to a geometric one, it would be indicated by a tendency of the differences to rise with the magnitudes. Judged by this, test Numbers I, III and V, in the last table, are more or less arithmetical in their tendencies; Number II is very irregular, but can hardly be said to favor the geometric series; while Number IV does distinctly lean to the geometric. By a fortunate chance, Number IV is the only subject who appears in both experiments being the Number I of the "six division" series; and if we refer to that record, we find a very similar tendency there, though in the average it is entirely overbalanced by the "arithmetical" tendencies of the other three observers. We have thus indicated that whether or not the psychophysic law is obeyed in these experiments may be an individual matter. As a further test of this relation, I asked all of those who sorted the sticks into six magnitudes (as well as some of the others), after they had finished, to draw six lines of the lengths, equal to the average sizes of the magnitudes

[1] I shall not discuss the nature of these irregularities further than to emphasize the importance of the *number* of sticks in each magnitude upon the average length; the numbers are irregularly distributed, and it is very noticeable that so frequently when the number of sticks is very much larger, or very much smaller, than the average number, the average *length* of these sticks also deviates from the usual average. Again, the first and the last magnitudes are apt to be irregular, because all very small sticks go into the one, and all very large ones into the other, and the number of such sticks present will evidently affect these averages. When a large number of sticks is placed in the smallest magnitude, its average will be high, and the reverse is true for the highest magnitude. A similar effect is noticeable in star-magnitudes, for which see my paper as above cited.

which they had in mind when sorting the sticks. These estimations agree as well as could be expected with the results of measurements, both in the average (which I here append)

Lengths of lines:	47.5	82.5	120.0	156.7	193.5	244.7

and in the individual records, the subject with the distinct "geometric" tendencies also revealing this trait in the lines he drew. This would indicate a rather more definite and conscious representation of the several standard magnitudes than I for one should have anticipated.

To express the degree of approach of the average results in the two sets of experiments to an arithmetical series, I append these averages, together with the ideal series, to which they most closely approximate:

Real Series,	31.6	78.4	128.0	181.8	226.2	269.8
Ideal Series,	32.1	80.3	128.5	176.7	224.9	273.1

Real Series,	44.9	65.2	84.2	108.7	134.9	166.1	201.2	231.4	250.3
Ideal Series,	35.4	62.3	89.2	116.1	143.0	169.9	196.8	223.7	250.6

We can further express the average deviation of the actual from the ideal series as a percentage of the average lengths, and will find this to be 1.6% for the first set, and 3.8% for the second. These figures may be regarded as measuring the approximation of the result to an arithmetical series.

B.—Tactual-Motor Extension.

With the assistance of Lucien Mason Hanks and James Bremer Kerr.

The above mentioned experiments were made at the Psychophysical Laboratory of Johns Hopkins University, in the spring of 1888. In order to extend the application of the method, and to investigate whether the result would be the same with a less accurate sense, I decided to continue the study at my present laboratory by performing the same operation of sorting the sticks into six magnitudes, but with the difference that the sticks were not *seen* by the subject. The latter simply *felt* their lengths by moving his forefinger along them and announcing the compartments in which he wished them placed. Each was then thrown into the bag by an assistant, who also gave the subject the next stick he was to feel. The process is thus the same, except that this form of tactual-motor sensation takes the place of visual sensation. The test was made with four subjects. The range of sticks in length was a little narrower than with visual judgments (the longest stick being about 25 mm. shorter than the longest stick with visual judgments), and the number of sticks also smaller—about 360 against 500. The number of sticks and

their average length for each observer, and their average is as follows :—

I.	Number,	60	69	79	50	50	53
	Average length,	35.2	71.3	112.8	158.2	189.9	235.1
II.	Number,	67	81	69	68	48	27
	Average length,	57.0	76.1	118.6	170.6	206.4	244.3
III.	Number,	55	64	69	66	50	54
	Average length,	35.1	67.5	100.6	148.6	190.6	238.6
IV.	Number,	60	35	56	57	52	97
	Average length,	37.1	63.2	85.9	117.3	162.6	224.0
Av.	Number,	60	62	68	60	50	58
	Average length,	36.1	69.5	104.5	148.7	187.4	235.5

The ideal series, to which the average of the four results approximates, is 40.55, 70.45, 110.35, 150.25, 190.15, 230.05, the average deviation of the two series expressed as a percentage of the average length being 2.6%. With regard to the individual records nothing requires special mention, except the fact that Number IV shows a tendency to follow the geometric series, especially so if we take into account the error in the average length of the lowest magnitude due to its being the lowest. In brief, the result is in every respect essentially similar to that with visual magnitudes, and all that has been said of the latter applies with equal force to the former.

The nature of the result being thus clear, I will at the present time offer nothing more than a few thoughts in explanation of the holding good of the law with star-magnitudes and its failure with extension magnitudes. The two queries that these results suggest are: With regard to what class of sensations can the psychophysic law be expected to hold good? And may the agreement with the law depend upon the method by which it is tested? Respecting the former it seems to me that the law includes such sensations as are appreciated *en masse*, and with not too distinct a consciousness of their intensity; when the sensation is a sort of impressionist reception of the gross sensation without dividing it up into units, or conceiving it as so composed, we may expect the law to hold good. This would be the case with the rough estimations of star brightnesses. On the other hand, when the impression is consciously received and definite in extent, as with spacial relations, the correspondence of the arithmetical with a geometrical series can not be expected, for if I am asked to draw a series of equally different lines, or if I am asked to sort sticks into groups, I have in mind the division of the range into *equal* groups, and I cannot help asking myself whether these groups are to be equally different *absolutely* or

relatively. The former seems to be the simpler and more natural conception, and it is accordingly adopted, whenever the problem becomes a conscious one; that this is what the subject has in mind, is clear from the lines he draws as the equivalents of his average magnitudes. Again, the individual who follows the geometric series would be one who did not consciously state the problem to himself, but went on a general impressionist view of the matter. At present this is offered merely as a suggestion that brings harmony into the results and emphasises the important part played by consciousness in the estimation of sensations. With regard to the second question I desire only to bring it into relation with the first, by calling attention to the fact that the psychophysic law seems to hold good of this class of extensive sensations when tested by other methods, and that therefore possibly a difference in the mental attitude of the subject may decide whether the sensation will be perceived under the psychophysic law or not. Apart from the interest in the experiments as an extension of a psychophysic method to new fields, these are the points of view from which I trust the present research may be of interest.

THE PERCEPTION OF SPACE BY DISPARATE SENSES.

With the assistance of FREDERICK WHITTON.

In a paper under this title, published in *Mind*, XI., No. 44, I offered the following as a provisional, but perhaps convenient, classification of the avenues by which we could gain knowledge of spacial relations:—

"I. By the stimulation of a definite portion of a sensitive surface:

(1) Of the retina (where the distance of the stimulating object must be inferred.)

(2) Of the skin.

(*a*) By the application of a pair of points, leaving the intermediate skin unstimulated, or

(*á*) Stimulating it by the application of a straight edge.

(*b*) By the motion of a point along the skin (see *Mind*, 40, pp. 557.)

[(*a*) and (*b*) may be contrasted as simultaneous and successive.]

"II. By the perception of distance between two movable parts of the body, *e. g.*, between thumb and forefinger.

"III. By the free motion of a limb, *e. g.*, the arm."

I then proceeded to investigate in detail the space relations furnished by a variety of I (1), of II and of III, deducing a

series of relations in part to be referred to in the present study, but for a full account of which the reader must have recourse to the original memoir. In the present study a form of I (2) (*a*) was interpreted by drawing lines with the aid of the eye, in which process the eye is naturally the guiding sense.

The method of work was as follows: Two spots were marked upon the volar surface of the forearm of each arm, one near the elbow, the other near the wrist. One of a pair of points was applied either to the lower (near the wrist), or the upper (near the elbow) of these points, and the other at various arbitrarily selected distances from the former. It goes without saying that the subject was prevented from seeing the pair of points applied to the arm by the interposition of a screen. Ten observations were made in a set, keeping one of the points constant throughout. The subject appreciated the distance between the points, and drew with a pencil a line, the length of which seemed to his tactual sensation, (not to his judgment or actual knowledge of the relation,) equal to it.

Even this was a difficult task, owing to the coarse sensibility of the forearm, and the estimations were made with little confidence and much hesitation and fluctuation. Owing to this, it was allowed to have the points applied (for a moment only) as often as the subject required, and he could correct and recorrect the lines drawn, until he felt satisfied with the result. Again, the arm fatigues very easily, especially at and near the point under constant stimulation, this being mainly due to the rather strong impression of the points necessary to give a distinct sensation. The apparatus employed was the æsthesiometer I described and figured in the proceedings of the American Association for the Advancement of Science, 1887, and also partly in this JOURNAL, Vol. I, p. 552.[1] I again take the opportunity of gratefully acknowledging the grant made me by the "Elizabeth Thompson Science Fund," by aid of which this apparatus was constructed.

The smallest lengths applied were determined by the smallest distance between the two points still felt as two; the largest by the dimensions of the forearm. Four cases were distinguished, according as (1) the right or (2) the left arm was

[1] The only change made was in setting the bar bearing the points upon adjustable brackets projecting at right angles from the uprights, to enable the arm to rest more conveniently beneath it. I will not describe the apparatus further than to remark that it offered great facility in leaving the operator both hands free for work, in applying both points equally well and always in the same way, and in making the setting and recording of distances extremely easy. The only difficulty is in the disposition of the arm to give both ease of application and comfort.

used, and as (*a*) the upper or (*b*) the lower point was kept constant, the latter distinction is necessary, because the sensibility differs at the two points. This was tested as a rule both before and after each set of ten observations; it being found that the fatigue incident to the experiments diminished the sensibility. The results of these observations are embodied in the following table:

J. JASTROW.	RIGHT ARM.		LEFT ARM.	
	UPPER CONSTANT.	LOWER CONSTANT.	UPPER CONSTANT.	LOWER CONSTANT.
Before	58.0	31.7	57.4	33.0
After	68.8	46.0	73.2	42.5
Average	63.4	38.9	65.3	37.8
F. WHITTON.	RIGHT ARM.		LEFT ARM.	
	UPPER CONSTANT.	LOWER CONSTANT.	UPPER CONSTANT.	LOWER CONSTANT.
Before	52.3	32.2	64.8	37.2
After	64.0	41.7	77.0	51.0
Average	58.2	37.0	70.9	44.1

The numbers express in millimetres the distances between two points just felt as two. It would be fairest to consider the average sensibility throughout the experiments as the mean of the sensibility before and after, and this is accordingly added in the table. The table shows: (1) That the sensibility at the lower point (near the wrist) is finer on both arms and for both observers than at the upper point (near the elbow), and on the average the points are perceived as distinct when 25 mm. nearer. (2) That the average just perceptible distance is for the upper point 64.5 mm., for the lower 39.5 mm. (3) That for Mr. Whitton the right arm is more sensitive both above and below than the left, while no such difference is apparent for myself. (4) That the effect of the fatigue increases the just perceptible difference after ten observations on the average by 12.2 mm.

As regards the chief object of the investigation, I have in the following table divided the observations into five groups, aiming to have the averages of the groups separated by about equal intervals, and have placed under each average distance between the points, as applied upon the forearm, the average length of the lines by which it was represented, and under this in turn the ratio of the two expressed as a percentage. This is done separately for Mr. Whitton and myself, and with the distinction of the four cases as already noted.

J. Jastrow.

	Right arm: Upper point constant. 1.	2.	3.	4.	5.	Right arm: Lower point constant. 1.	2.	3.	4.	5.
Real length . . .	64.0	80.0	101.2	120.0	139.5	59.4	79.0	101.7	121.9	145.0
Drawn length . .	15.0	19.7	25.2	38.1	56.4	17.6	23.0	32.7	42.9	53.8
Ratio in percentage	23.4	24.6	24.8	31.7	40.4	29.6	28.1	32.1	35.2	37.1

	Left arm: Upper point constant. 1.	2.	3.	4.	5.	Left arm: Lower point constant. 1.	2.	3.	4.	5.
Real length . . .	63.0	80.4	102.3	123.0	138.0	60.4	81.3	100.8	121.5	141.0
Drawn length . .	12.7	17.8	29.8	43.7	56.1	14.9	23.0	32.1	41.9	61.4
Ratio in percentage	20.2	22.1	29.1	35.5	40.6	24.7	28.3	31.8	34.5	43.6

F. Whitton.

	Right arm: Upper point constant. 1.	2.	3.	4.	5.	Right arm: Lower point constant. 1.	2.	3.	4.	5.
Real length. . . .	64.2	82.7	101.2	120.8	138.3	55.6	79.2	99.8	120 0	137.5
Drawn length. . .	42.8	51.1	70.6	79.2	87.7	39.3	58.2	74.8	82.1	98.8
Ratio in percentage	66.7	61.8	69.8	65.6	63.5	70.7	73.5	75.0	68.4	71.9

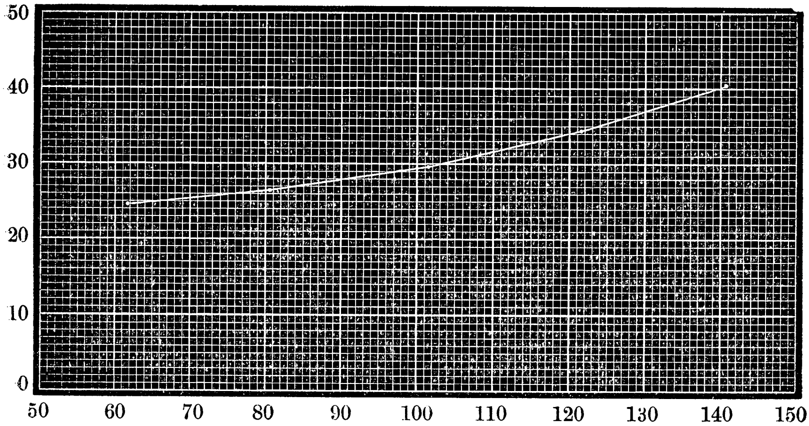
	Left arm: Upper point constant. 1.	2.	3.	4.	5.	Left arm: Lower point constant. 1.	2.	3.	4.	5.
Real length. . . .	65.0	82.0	102.5	121.7	141.2	57.7	80.3	100.7	117.5	129.6
Drawn length. . .	30.0	42.5	60.5	75.8	97.0	39.7	59.9	70.0	88.5	97.6
Ratio in percentage	42.2	52.0	59.0	62.3	67.2	68.8	74.6	69.6	75.3	75.3

These Tables show: (1) *That the lengths are all very much underestimated*, the lines being on the average 65.6% of the distances between the points for Mr. Whitton and but 30.9% for myself; (2) That for myself throughout *the underestimation decreases as the length increases*, though for Mr. Whitton this is true in one of the four cases only; (3) That the underestimations are less when the lower point than when the upper point is constant, on the average by 7.2 mm. for Mr. Whitton, by 3.9 mm. for myself; (4) That for myself there is no difference between the sensibility of the two arms, but for Mr. Whitton the right arm is slightly more sensitive than the left.

Postponing the further discussion of these results, I will assume that the average sensibility along the arm is that midway between the sensibility at the upper and at the lower point, and that there is no difference in sensibility between the two arms; I then take my own result as rather the more regular of the two and obtain the most typical result by combining the four cases for myself as is done in the following table:

Real length,	61.7	80.2	101.5	121.6	140.9
Drawn length,	15.1	20.9	29.9	41.7	56.9
Ratio in per cent.,	24.4	26.1	29.5	34.2	40.4

The same is expressed graphically in the accompanying curve.[1]



I shall now discuss the relation of this result to the conclusions of my former paper. Such discussion can only include the most general relations, a minute comparison being impossible owing to the difference in the number of subjects and observations. The most general conclusion of my former paper here pertinent is that "*If the eye is the expressing sense all lengths are greatly underestimated*, the error decreasing as the length increases.[2] With this general result this curve is entirely in agreement, although the decrease of the error with the increase of the length is not as marked, owing in part to the smaller range of lengths that the observations cover. Regarding the comparative accuracy of the feeling of tension between thumb and forefinger, the motor sensations of the arm and the skin sensibility of the fore-arm, accurate statement is impossible, but the indication is that the last is a less accurate source of space-perception than either of the others. My general result is thus an additional verification of the conclusions reached in my former study, and an extention of their significance. The space-perceptions of disparate senses are themselves disparate, and whatever harmony there is among them we are warranted in regarding as the result of experience. The

[1] The ordinates express the drawn lengths as percentages of the real lengths, the latter being indicated by the abscissæ.

[2] Though the method of expressing by the eye is different here from what it was in the former study (no pains being taken to restrict the movement of the eye and the hand moving over the space drawn). I do not think it likely that this difference at all seriously influenced the results owing to the supremacy of the eye in all spacial judgments.

spacial notions of one deprived of the sense of sight and reduced to the use of the other space-senses must indeed be different from our own. And the existence of the striking disparities between our visual and our other space-perceptions, without confusing us, and, indeed, without usually being noticed, can only be explained by the tendency to interpret all dimensions into their visual equivalents and unconsciously correct them by the same means. The general law to which the result contributes seems to establish a sort of co-efficient of conversion; the same amount of objective stimulation upon a delicately sensitive surface is interpreted subjectively as the equivalent of a much more extensive sensation than an equal objective stimulation upon a coarsely sensitive sense-organ. There is, as it were, an exchange of the spacial units of different senses, and because the visual units are the smallest it takes a smaller visual space to seem equal to a larger tactual or motor space.

A few points peculiar to the present research remain to be noted. The first is the peculiar fact that when the points are extended a few millimetres there is a sort of jump from the point at which no interval at all is felt (the two points being felt as one) to the perception of the entire interval. The moment we perceive an interval at all, we regard it as longer than the mere separation of two points; it is not that the zero point is at a constant height, but that the sensation changes its character. To my knowledge the theory of dermal sensibility is too little advanced to give an adequate explanation of the fact, nor have I any to offer. The fact itself seems to me important, and must be accounted for by any theory that claims general acceptance. A second point is that while the sensibility at the upper and at the lower points differs by about 25 mm. the difference in their reproductions is only about 5.5 mm. Even if we regard this difference as subject to the same underestimations as the absolute lengths it is strikingly small; but the explanation of the fact is even more difficult than of the foregoing.

ON THE PRESSURE SENSE.

With the assistance of SARAH BELLE FLESH and HELEN SMITH.

The problem set proved a much broader one than could be profitably worked out in the limited time at the disposal of the experimenters, so that only two aspects of the work can be here described, both of these relative to the methods of testing sensibility. The apparatus used for testing the pressure-sense was a modification of a Fairbanks' post-office balance, in which the initial and incremental weights were

placed upon the scale-pan, thus exerting an upward pressure upon the finger situated at the end of the beam. A series of attachments were added by which the pressure could be instantly released from the finger and thus the ill effects of fatigue averted. A comfortable and firm position of the arm, hand and finger was also secured. To obtain a normal sensibility, experiments were made according to the method of right and wrong cases, the subject being requested to answer each time, and doubtful answers being excluded so that half the answers would be correct by chance. At the bidding of the subject a pressure was brought to bear upon the finger; at a second signal the pressure was increased or diminished, and at a third the original weight was restored. The subject had to decide whether the middle pressure was lighter or heavier than the extremes. The two initial weights applied were (A) 315 and (B) 105 grms., and the changes were an increase or decrease by (1) $\frac{1}{7}$ or (2) $\frac{1}{21}$ of these weights. An attempt was also made to record the confidence[1] in the correctness of one's answer on a scale in which 3 signified relative certainty, 0 no preference for one answer above its opposite, and 1 and 2 intermediate grades of feeling. After throwing out certain observations made under distracting circumstances there remain 100 observations for each observer under each of the four cases. These are given in the table, together with the theoretical ratio at which according to the formula given in my paper published in this JOURNAL (Vol. I, p. 308), one-fourth of the answers should be correct.

MISS SMITH.

Initial weight.	Ratio of increment.	Percentage of error.	Ratio at which 25 per cent. errors would occur.	Average confidence
315 grammes	$\frac{1}{7}=1.143$	4.0	1.053	1.22
315 "	$\frac{1}{21}=1.048$	19.0	1.037	0.60
105 "	$\frac{1}{7}=1.143$	3.0	1.049	1.17
105 "	$\frac{1}{21}=1.048$	20.0	1.038	0.60

MISS FLESH.

Initial weight.	Ratio of increment.	Percentage of error.	Ratio at which 25 per cent. errors would occur.	Average confidence
315 grammes	$\frac{1}{7}=1.143$	10.0	1.073	0.78
3I5 "	$\frac{1}{21}=1.048$	34.0	1.080	0.54
105 "	$\frac{1}{7}=1.143$	12.0	1.077	1.14
105 "	$\frac{1}{21}=1.048$	40.0	1.132	0.62

The constancy of these numbers measures the constancy of

[1] This method was used in the research by Mr. Peirce and myself on "Small Differences of Sensation," Memoirs of the National Academy, Vol. III, and also in the paper in *Mind*, No. 44.

the sensibility as well as the agreement of the results with the requirements of the psychophysic law. The law seems approximately adhered to, though with variations depending largely on the small number of observations. The average ratio at which 25 per cent. of errors should occur is for Miss Smith 1.044, for Miss Flesh 1.090, the mean of which is 1.067; and as this measures the most probable error we in a certain sense express the fineness of the pressure sense as here determined, by saying that its probable error is 1.067 or about $\frac{1}{15}$.

A second series of observations was made under the same conditions except that instead of applying and removing the additional weight while the initial weight is upon the finger, the initial weight is applied and removed; then the initial plus or minus the additional weight is applied and removed; and then the initial alone again. The question is whether we can compare more accurately the change of a sensation x with the sensation $x \pm a$ (produced by simply adding or subtracting a), or the entire sensation x with the entire sensation $x \pm a$. The result for Miss Flesh is too much affected by what must be accidental errors to be here cited, but for Miss Smith it is as follows; the result is arranged as in the preceeding:

Initial weight.	Ratio of increment.	Percentage of error.	Ratio at which 25 per cent. errors occur.	Average confidence.
315 grammes	$\frac{1}{7} = 1.143$	11.2	1.077	0.27
315 "	$\frac{1}{21} = 1.048$	30.0	1.062	0.11
105 "	$\frac{1}{7} = 1.143$	12.5	1.081	0.27
105 "	$\frac{1}{21} = 1.048$	31.6	1.068	0.00

We see that this second method is decidedly the more difficult, the average "probable error" rising, for Miss Smith, from .044 to .072. The psychophysic law is well supported, though here as before the subject appreciates differences of $\frac{1}{21}$ relatively better than differences of $\frac{1}{7}$. Regarding the causes of the increased difficulty of the second method of experimentation it may be in point to note that memory has a wider play in it than in the former method, though this is not the entire psychological difference. The result shows too, how essentially tests of sensibility are dependent upon the methods employed.

Regarding the confidence we see that it rises as the proportion of error decreases and falls as this proportion increases; what this relation is I have no means of determining, nor do I think that it is constant or anything more than a subjective but practically useful aid in judging the reliability of the results.

On Just Observable Differences.

With the assistance of AUGUSTA ADRIENNE LEE.

The usual applications of the method of the Just Observable Difference aim to fix by more or less direct means the point at which two sensations are sufficiently different to have that difference consciously perceived when the attention is directed to it, and to arouse some confidence in the correctness of one's judgment of this difference. I have elsewhere[1] pointed out the uses and the abuses of this method and will here confine myself to the description of a hitherto unnoticed mode of testing the Just Observable Difference. A distinction, the importance of which is not always recognized, is that between the power to tell that two stimuli are different and the power to tell the direction of this difference. In some cases the later is always given with the former, but in others it is not. A great many persons can tell that tones are different without being able to tell which is higher and which lower. It matters much, too, whether the two stimuli are successive or simultaneous; and in the estimation of spacial relations it is important whether the two stimuli are placed side by side, so that their relations are manifest, or not. The form of the method now to be described is certainly a useful variation of it, and yet as far as I know has not been employed. It consists *in having the subject produce a stimulus just longer (more intense) or just shorter (less intense) than a given stimulus;* instead of judging differences presented to him he produces the smallest difference that he can. By this method a knowledge of the direction of the difference is made necessary.

In the first series of experiments fifty lines were drawn, their lengths varying in an arbitrary manner from about 25 to 150 mm.; and after viewing one line it was covered over, and the attempt made to draw a line just longer than the one seen. In the next series the attempt was made to draw the lines just shorter than the original lines; and in a third series (in order to eliminate a constant error, if there be any), the attempt was made to draw the lines just equal. In another set of experiments the same three processes were repeated, but the original line was kept in sight while the second was being drawn, though the two were kept at some distance apart so as not to make a fitting of the ends of the lines possible. The average number of millimetres by which a line differs from the original line under the three cases and when the original line was visible or not is given in the following table. I also give with this, the ratio of the average length of the

[1] See this JOURNAL, Vol. I, pp. 273-277 and 299-302.

line to this just perceptible difference expressed as a percentage.

	ORIGINAL LINE NOT SEEN.	
Just longer.	Just equal.	Just shorter.
2.17 mm. = 2.75 %	0.73 mm. = 0.92 % (Total error = 1.20 mm.)	2.50 mm. = 3.60 %

	ORIGINAL LINE SEEN.	
2.56 mm. = 4.01 %	0.36 = 0.56 % (Total error = 2.04 mm.)	3.78 mm. = 4.97 %

The conclusions that I draw from these results are: (1) that the error when the two lines are seen is less than when not (the case when the lines are drawn equal is no exception if we count as we ought the absolute error positive and negative; these cancel one another in the latter case and so give an appearance of greater accuracy); (2) the just perceptible difference is greater in drawing the just shorter than the just longer lines; (3) the error in drawing lines equal is quite small, and its effect upon the other results not marked enough to appear in these few observations; (4) the just perceptible differences are considerably larger than those found with the more usual method. This last I would bring under the general law that our powers of execution fall short of our powers of discrimination. If the psychophysic law is true it would appear in this method in the fact that the just perceptible difference would bear a constant ratio to the length reproduced. If I divide the lines into short, medium and long lines, I get three just perceptible differences that are approximately constant ratios of the average lengths. I desire here mainly to call attention to this psychophysic method as a natural and easy method of obtaining a reliable quantitative result, and one easily comparable with the results of other methods.